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sea, must either have never spawned or cast its spawn into the river.

The larvæ of eels as well as of some other soft-rayed species are quite pellucid, without pigment cells and with "a roomy space between the skin and the muscles, distended by a watery fluid." Many of these larvæ, in their transformation to the condition of young fishes become much reduced in size, though increasing in weight, by the obliteration of these interspaces.

Professor Meek's studies pass through the whole long series of fish-families. For want of space, we may not follow them further in these pages. We must give the work, as a whole, very high praise as carefully, intelligently and scientifically done, and as constituting a reference book of great value. The author has well covered the range of the periodicals which treat of the distribution and habits of fishes. He seems, however, to have overlooked the most extensive recent work of a similar range, "*Jordan's Guide to the Study of Fishes*," published in New York in 1905.

DAVID STARR JORDAN

NEW LIGHT ON BLENDING AND MENDELIAN INHERITANCE

UNDER the above heading, Dr. Castle reviews a paper by Yuzo Hoshino on the inheritance of the flowering time in peas and rice.

Since reading this review, Prof. Hoshino kindly sent us his paper, and we have ourselves examined it with care to see whether indeed it necessitates Dr. Castle's rather sweeping conclusions, namely, that certain genes are themselves modified by crossbreeding, one of the conclusions of Hoshino himself, and that selection within a pure line, within a genotypically pure population is effective.

It is well known that Dr. Castle counts among the few last geneticians, who still believe that the genes themselves are modifiable by selection. Hitherto in nearly all his writings on the subject Dr. Castle claimed, that unit characters vary, and may be modified by selection, a statement which can not very well be opposed, given the loose way in which the obsolete term unit character is usually applied. But it was clear, that Dr. Castle really believed the genes themselves to be capable of variability in potency, quality and value, and we think it of the utmost importance that in the review under discussion he has stated the

question in these words. Thus the issue between Dr. Castle on one side, and Johannsen and us on the other narrows, and there need be no more difficulty as to the exact meaning of the term unit character. As to the effectiveness of selection in genotypically homogeneous material, all the evidence so far adduced shows that selection in such material is absolutely ineffective. It is evident that selection in a population is usually effective, but this only shows that in ordinary populations, even in so-called pure strains of animals, there is a good deal of genotypic variation, or in other words impurity.

The fact, for instance, that Dr. Castle's selection in hooded rats was effective, shows that his material was not originally pure for all the genes. In all those instances where the guarantee for genotypic purity of the material was reasonably good, selection has, until now, proved ineffective. We need only point to the fifty years of selection in wheats by the de Vilmorin family, and to the numerous selection-experiments with clones of *Paramœcium* by Jennings and others.

As to the so-called instances of the effectiveness of selection on the genes themselves in alleged genotypically homogeneous animal material, we repeat that the only way to show such an effect in material which offers no sure proof of purity would be to change a strain of severely inbred animals by selection to a point removed from the range of the ordinary modification in the material, continuing the inbreeding, and then, by contraselection, to bring the character under consideration back to its starting-point. Since we wrote down this challenge to the believers in the variability of genes, one such a series of selection-experiments has been performed, namely, on flies, and in this series it has been proved to be impossible to get the material back to its original quality.

According to Dr. Castle, Hoshino's Table 6 shows the effect of selection within a pure line. In the cases taken from Hoshino's paper, in which the progeny of an early-flowering and a late-flowering individual of the same "pure line" can be compared (in the original table there is one more case in which the earliest parent gives the latest progeny) there are more instances in which an early parent gives an earlier progeny than a late parent, than cases in which an earlier parent gives a later progeny. But if we examine the figures more closely, we observe that the mean deviation of offspring from parents in the case in which the earlier parent gives the earlier progeny is 0.52 day,

whereas the mean deviation of offspring from parent in cases in which the earlier parent gives the later progeny is larger, 0.65 day. The only conclusion from Hoshino's Table 6 is the one he makes himself, namely, that the variation is insignificant.

We are sure that no unbiased person would conclude from the negative facts in the table in question that the variation in these pure lines was genotypic, or that selection in these groups has had an appreciable effect.

On page 332 Dr. Castle writes:

If I have correctly interpreted Hoshino's observations, flowering time in peas is clearly a Mendelian unit character, entirely devoid of dominance, so that a strictly intermediate hybrid form is the commonest end product of a single cross between early and late varieties.

Indeed, if Hoshino's work on the inheritance of flowering-time of peas were the only, or the first, or the most comprehensive of its kind, we could see reasons for such a belief. But Hoshino only crossed two varieties differing in time of flowering. But in peas there do not exist only one late and one early variety, but several thousands, each with its own time of flowering. It would not be difficult to give a list of ten names of pea-varieties of which in every preceding one all the plants would be in flower before one of the next opened its first flower. Therefore crossing experiments involving two varieties can never be sufficient basis from which to conclude that flowering-time in peas is one thing or the other.

Tschernack, in his well-known experiments with flowering-time of peas (1911, Mendel's Festschrift), cited by Hoshino, made eight different variety-crosses. Whereas in Hoshino's work the two varieties crossed happened to be of such a constitution, that in the resulting F_2 generation there did not occur plants which commenced flowering at an earlier time than the earliest parent, or at a later date than the latest parent, in Tschernack's work such cases were met with. In Tschernack's experiment No. 81 (1906) there were in F_2 found plants flowering seven days earlier than the early parent; in experiment No. 82 (1916) even plants beginning flowering nine days earlier than the early parent. In experiment No. 81 (1906) there were also found plants starting to flower four days after the latest parent, and in experiment 38 (1902) there were plants, which did not begin to flower before the late parent had been in flower for a week.

It is perfectly clear, that a sort of blending may be the result of a difference between the parents in a number of genes, influencing the quality under observation in different directions.

On page 333 Dr. Castle writes:

In typical blending inheritance the determiners of contrasted parental conditions apparently blend into a determiner of intermediate character, the gametes formed by an F_1 individual being practically as uniform in character as those of either parent. Blending is illustrated in the inheritance of ordinary size-differences in birds and animals.

No one who knows the work of Punnett and Bailey (cited in Hoshino's paper) on chickens, in which they found not only individuals in F_2 as small as the smallest parent and as large as the largest, but even individuals lighter than the lightest parent and heavier than the heaviest, could maintain that ordinary size-inheritance in birds is blending. The gametes formed by the Hamburg \times Sebright hybrids, or by our Leghorn \times fighting bantam certainly were not as uniform as those of any of the four parental strains!

We are perfectly in accord with Castle when he reasons that if once we admit a contamination of genes and qualitative changes in genes, we do not need to assume that flowering-time in peas is influenced by two genes, in the cases studied by Hoshino. In such a case the difference in one gene would suffice. Indeed, we would go one step farther than Castle and declare, that, on the assumption of qualitative changes in genes, we need not assume a genotypic difference between the parent varieties at all. Where we differ from Dr. Castle is in the fact that we do not believe in qualitative variation of genes. Surely more than ten genes must influence the beginning of flowering in the pea, else there could not be so many varieties differing in the time of flowering. All the genes which influence stature, shape of flowering axis, color, must necessarily influence the onset of flowering. And we need not look for coupling between color factors and flowering-time factors, because the factors influencing color influence the metabolism of the whole plant, and thus the period at which it starts flowering.

If we compare Hoshino's paper with Tschernack's extensive experiments on the subject, we find nothing in it, which would make us assume contamination of genes by crossbreeding, or any qualitative variability of genes.

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